V

MARRIAGE AND GONORRHŒA*

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WHEN the Council of the Medical Society for the Study of Venereal Diseases did me the honour of suggesting I should open the discussion on Marriage and Gonorrhœa, I was somewhat puzzled as to how I could approach the subject in such a way as to be interesting as well as informative—for, stripped of all redundancies, the question resolves itself into a bald categorical enumeration of the tests that can be applied to either sex to determine when any particular individual, who has suffered from gonorrhœa, has reached the stage when he or she can be pronounced non-contagious in sexual intercourse.

Thinking it over, it occurred to me that the most stimulating way one could deal with the matter would be to give a brief summary of the historical views held during the past 300 years upon the nature of the disease, its infectivity, treatment, and the standards of cure recognised from time to time. The ground being thus cleared, one could then contrast these older views with the standards accepted at the present time. Many of these former opinions have naturally fallen into the limbo of things forgotten, but some persist obstinately amongst laymen even to-day; and it is an instructive lesson in humility to find that quite a number of the ideas we now condemn so heartily are actual survivals of opinion held by the leaders of the medical profession even as late as the eighteen-eighties.

Gonorrhœa apparently existed in the Old World long before historical times. The fifteenth chapter of the Book of Leviticus summarises the tests for cure imposed by Moses, and there are prescriptions in the Ebers Papyrus which suggest the ancient Egyptians suffered from the disease. The invention of the name "*gonor-*

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rhœa " is generally attributed to Claudius Galenus, the celebrated Greek physician, who flourished in Rome A.D. 164–200. He taught that the discharge in the male was caused by a flow of diseased semen from the vesicles and prostate, and does not seem to have recognised the condition in the female at all. So great was the weight of tradition behind his name that this belief, as to the origin of the discharge, was still held firmly in the reign of George I., for we find that *William Cockburn* considered it necessary to devote two whole chapters of his book [1] to refuting the idea, maintaining that gonorrhœa started in the urethra, and that the discharge was not the result of pus coming from an internal ulcer, but due to an inflammatory reaction of the mucous glands of Littré.[2] In addition, he advanced the thesis that gonorrhœa could be cured by injections into the urethra, and that bleeding was unnecessary. All this was entirely unorthodox, and drew forth much abuse from his contemporaries, particularly when he refused to disclose the ingredients of the injection which he praised so much. We thus find Astruc, in his great work on Venereal Diseases published in 1737,[3] deriding him, and repeating all the old fallacies, although he was a man of profound learning and great critical acumen. To him gonorrhœa was still a flow of diseased semen, and pus evidence of the presence of an internal ulcer. Astruc, like Galen, triumphed ; the whole profession followed him like sheep ; and it remained finally for John Hunter to disprove both these age-long fallacies. Hunter's doubts arose in the following way: In 1749, on doing a post-mortem on a child with empyema, he noted to his surprise that the surface of the pleura, when the fibrin was rubbed off, remained intact, and so came to the conclusion that " *matter could be formed without a breach of surface.*" As he was already very interested in venereal diseases, he therefore thought of investigating what actually happened in gonorrhœa. " So much being known," he writes, " I was anxious to examine whether the matter in a gonorrhœa was formed in this way. In the spring of 1753 there was an execution of eight men, two of whom, I knew, had at that time severe gonorrhœa. Their bodies being procured for this particular purpose, we were very accurate in our examination, but found no ulceration. The two urethras appeared a little bloodshot, especially near the glans." [4]

Identity of Syphilis and Gonorrhœa

I dwell on this theory of the presence of an internal ulcer in gonorrhœa because it was owing to the prevalence of the belief that the great heresy as to the pathology of the disease originated—a heresy which vitiated treatment for over 300 years, a heresy, moreover, which Hunter himself, in spite of his discovery, did much to keep alive. This heresy was the belief that syphilis and gonorrhœa were the same disease, the ulcer being inside in gonorrhœa and outside, as a chancre, in syphilis. All through the Middle Ages gonorrhœa was a well-recognised entity. Its contagiousness was known; its venereal nature appreciated. Many regulations were made to check its ravages, Wm. Becket,[5] in the Philosophical Transactions of London, 1718, cites the regulations for the Stews in the Borough of Southwark promulgated in 1430 in support of this.

The Outbreak of Syphilis

But when the great epidemic of syphilis broke out at the end of the fifteenth century, the new disease so overshadowed the older one that nearly all that was known about it was forgotten. Syphilis was introduced into Europe by Columbus' men returning from the West Indies in 1492–93, and burst forth in epidemic form after the siege of Naples in 1494–96. So speedy was its progress that it had spread all over Europe before the beginning of 1500. At first the two diseases were quite well differentiated, but about 1530 *Paracelsus*, in his "Chirurgia Magna,"[6] announced his opinion that the discharge in gonorrhœa was only a preliminary symptom of syphilis, and though many scoffed at him he found a warm supporter in the great military surgeon, Ambroise Paré, who strongly advocated this view.[7] The heresy soon spread all over the continent, but was not so readily accepted in England, where the "French disease," "Morbus Gallicus," or "French Pox," as syphilis was then called, had been recognised as a foreign importation quite distinct from the older and already familiar disease, gonorrhœa. Nevertheless, the opinion grew, and eventually became more and more firmly rooted, until it was accepted as an axiom by almost every one of weight in the profession—Sydenham, for instance, held it without reserve.[8]

Gonorrhœa, therefore, now assumed a new importance ; and from being considered, as it had been, a local disease, a trivial inconvenience, came to be looked upon as the forerunner of the most dreaded malady of modern times. In England, as I have said, the view was never accepted with the whole-hearted belief of the Continent. It was noted that many cases of gonorrhœa got well under local treatment, without any of the constitutional symptoms of syphilis, and the profession compromised, therefore, by recognising two types : simple gonorrhœa, which cleared with medicines and injections, and malignant gonorrhœa, which turned to syphilis. The connection between the two was explained by the theory that, if the discharge was cured too quickly by astringent medicines or injections, the purulent matter of the internal ulcer could be driven into the system and then show itself as the constitutional complaint.[9]

The Duality Theory

Such was the state of medical opinion when *John Hunter*, to settle any remaining doubts, made his famous experiment to prove or disprove what was called the duality theory still held by certain persistent people. In May, 1767, he made two punctures with a lancet, one on his glans, the other on his prepuce. These punctures he inoculated with pus from a supposed case of gonorrhœa, and in each of the punctures a chancre developed, which was followed later by secondary symptoms.[10]

This disastrous experiment captured the belief of the entire scientific world, and although Benjamin Bell repeated it in 1792 on three medical students who developed gonorrhœa only, Hunter's views carried the day for over forty years, and practically every case of gonorrhœa was treated with mercury. Hunter himself cannot be accused of starting this line of treatment—it had been practised long before his time—and he definitely stated,[11] " one medicine, that is mercury, cures only the chancre and the lues venerea, and the gonorrhœa is not in the least affected by it." But his disciples would have none of this caution, and so, more zealous than their master, persisted in treating every case of gonorrhœa with a full course of mercury. What this meant only those acquainted with the history of the treatment of syphilis

can appreciate. Individual surgeons inveighed against it, but the custom still persisted well into the nineteenth century, for we find Sir Astley Cooper, in his lectures (1824–27), attacking it as follows :—

" No greater folly, or indeed cruelty, can be committed than that of giving mercury for the cure of this disease ; a man must be grossly ignorant or shamefully negligent of the duties which he owes to the character of his profession, and to the common dictates of humanity, if he persists in doing so. I scarcely ever enter the foul wards, because patients are compelled to undergo so infamous a system of treatment that I cannot bear to witness it. To compel an unfortunate patient to undergo a course of mercury, for a disease which does not require it, is a proceeding which reflects disgrace and dishonour on the character of a medical institution. If you go to a patient in the foul wards at the end of his course, and ask him how many times he has rubbed in, he will generally answer : ' Twenty-eight times.' If you ask him whether he is salivated, he will tell you that he spits three pints a day ; but ask him whether his gonorrhœa is cured, he will reply : ' No. I have a clap still on me.' " Sir Astley Cooper was surgeon to Guy's and lecturer on surgery at St. Thomas' Hospital when he penned these lines, so his remarks applied apparently to one or other, or both, of these great institutions.[12]

Seemingly feeling ran high on the subject about this time, for in the pages of the *Lancet* between 1830 and 1836 much acrimonious discussion took place. But opinion gradually hardened against the practice, and the profession, therefore, was ripe to accept Philippe Ricord's masterly exposure of Hunter's mistakes when it appeared in 1837. His experiments, over 700 in number, carried out between 1831 and 1836, satisfied the scientific world that Hunter was wrong ; and, as a consequence, the age-long fallacy of the identity of the two diseases was banished from medical literature.

But Ricord, as great an observer in his way as Hunter, too, made his mistakes. This is not the place to comment on the dangerous views he held concerning the infectivity of secondary syphilis. One should remember, however, that he taught that gonorrhœa was not conveyed by a specific virus, but was a simple catarrh, and could be caused by other irritants, including chemical ones, as well

as by gonococcal pus. In proof of this he stated that he had often examined women accused of giving gonorrhœa, and found many of them quite healthy, a statement previously made by Hunter. He also believed with Hunter that gleet was not contagious.[13] Ricord's views, like Hunter's, held the field for fully forty years, and were still the orthodox teaching when James Lane published his Harveian lectures in 1878.[14] Here we find him stating that, though gonorrhœa is generally caused in the male by contagion from the female, " yet this often happens when no disease whatever can be discovered in the female supposed to be at fault." Later on he adds, " I believe the great majority of vaginal discharges which may give rise to gonorrhœa in the male do not depend on contagion at all, but arise spontaneously as the result either of some constitutional or local disorder."

That Lane was not alone in his views is evidenced by the fact that Henry Lee, consulting surgeon St. George's Hospital, restated the same opinions in 1883,[15] quoting with approval Ricord's dictum. " Gonorrhœa often arises from intercourse with women who themselves have not the disease." [16] Such, then, were the opinions held by the leaders of the profession about the time of Queen Victoria's Jubilee, a time which some of us can remember quite clearly. It is little wonder, therefore, that the average practitioner paid little or no attention to the question of cure, and less still that the layman paid none at all. Gonorrhœa in the male by now had fallen from being the precursor of syphilis to a disease that could be acquired by over-indulgence in alcohol. Gonorrhœa in the female was still looked upon as a sort of vaginitis, for, though Lane describes quite accurately all the ordinary symptoms of acute and chronic endocervicitis, he missed the point that they were gonococcal in origin. His practice, however, as is often the case, was in advance of his teaching, for we find that at the Lock Hospital in 1868 he examined women with the speculum, and twice or three times a week treated the cervix with silver nitrate or alum or liq. ferri perchlor., using also tampons of cupri sulph. tannin, etc., in the vagina.[17] In his treatment, therefore, he was fully thirty years ahead of his time, for the general view was that if the urethra and bladder gave no discomfort, and there were no obvious lesions in the vagina, the patient was considered clear. Endocervicitis was

looked upon as cervical catarrh, and the connection between pelvic peritonitis and gonorrhœa was not recognised. Gleet in the male was still believed to be no bar to marriage, and Henry Lee was considered rather fussy because he thought it was.

The history of the very gradual growth of knowledge as to the condition in the female is singularly instructive. The disease must have been known to exist in women long before the earliest records of prostitution, because regulations for the examination and segregation of diseased women existed in full force in mediæval times, yet very little about the real nature of the contagion seems to have been suspected, and what was taught was generally wrong.

It is true Nicholas De Blegny (1673), in his well-known work,[18] maintained that the principal seat of gonorrhœa in women was the womb, but Astruc cast scorn upon him as a quack and an impostor,[19] Cockburn rejected him, and John Hunter stated positively,[20] " It has been asserted that the ovaria are sometimes affected in a similar manner to the testicles in men. I have never seen a case of this kind, and I should very much doubt the possibility of its existence." Ricord, however, in Paris, must have been teaching in 1836 that gonorrhœa could infect the womb, because in his well-known atlas he figures cases of uterine discharge, and his pupil, Acton, as early as 1841, is found giving a very accurate account of an attack of acute salpingitis as follows :—

" *Blenorrhagia of the Uterus.* There is one (complication) which we believe is new to English practitioners, at least we do not remember having read of it in English works. We allude to an ' *Ovitis* ' which bears an analogy to epididymitis in the male. Thus a female suffering under uterine blenorrhagia may be seized with a shivering, and a feverish state of the system ; vomiting may come on, together with a pain referred to the iliac fossa, where more or less tension may be present. Pressure on the os uteri gives no suffering ; but if a finger be carried up the cul de sac of the vagina, and the patient desired to turn upon the opposite side, pain of an acute kind will be felt. The blenorrhagia may cease for the moment, one ovary may be attacked only, or both simultaneously as in epididymitis. . . . We believe that a great number of ovarian dropsies may result from a chronic inflam-

mation of that organ, the consequence of such complications." [21]

Acton also gave a good clinical picture of the usual erosions, etc., seen on the cervix in gonococcal inflammation; but apparently his work seems to have been ignored in London, otherwise we could not have had such views as Lee's accepted in 1883.

Continental opinion generally seems to have been similar to that held in England; and it is to American surgery we are indebted for the views of the severity and intractableness of the disease in women that we now hold. The first American references I can find are those of Homer Bostwick,[22] in his work on Venereal Diseases published in New York in 1848. In this work he asserted that inflammation of the ovaries might exist as a complication of gonorrhœa, and also remarked that "when certain peculiar erosions or superficial ulcers may be seen on the mucous membrane covering the cervix uteri there is little doubt that the disease is a true gonorrhœa." Bostwick was a very acute clinician, but it is probable that he derived most of his opinions from Acton and Ricord, as in his book he pirated their illustrations practically without acknowledgment,[23] and, in consequence, his real contributions to the subject have been overlooked, for in the encyclopædic work of Norris [24] there is no reference to him. It is possible, however, that his work did bear fruit, for the name which is associated with the first real attempt to describe the true nature of the disease in women is also associated with New York.

That name is *Emil Noeggerath*, a German doctor who, after retiring from practice there, published his epoch-making book on "Latent Gonorrhœa in Women," in 1872.[25] In this he stated his conviction that the vast majority of cases of salpingitis, oophoritis, pelvic peritonitis, were due to gonorrhœa. Undoubtedly he exaggerated his case, like most enthusiasts; but in the main his thesis has been confirmed, though his gloomy prognosis as to the incurability of the disease in both sexes has never been accepted, and was, indeed, eventually abandoned by himself. His book, however, fell absolutely upon stony ground at the time it was issued. He was a prophet crying in the wilderness. The only reference to him in English for years was that of Angus Macdonald [26] in 1873, and, as we have seen, eleven years later, Lee, in

Holmes' Surgery, ignored him—that is, if he had ever heard of him at all. Probably he had not.

Discovery of the Gonococcus

It might have been thought that the discovery of the gonococcus by Neisser and Watson Cheyne simultaneously in 1879 (Watson Cheyne's work unfortunately did not appear until 1880 [27]) would have settled the question of the specific infectivity of the disease at once. But this was not so. Neisser's discovery at first raised very little enthusiasm, so many groundless claims having been made in the previous twenty years; and it was not until his statement had been confirmed from many quarters that any special notice was taken of it. Even then there were many doubters, and it was not until the cultivation of the organism, and its fulfilment of Koch's postulates, that these doubters were silenced. By 1890, however, no further doubt of the infectivity and the specific nature of the disease was possible. The idea that gleet was non-contagious went by the board, and the opinion that a woman could give a man gonorrhœa without herself suffering from the disease died a natural death. Gradually then it began to dawn upon clinicians that many manifestations, apparently unconnected with gonorrhœa, were the result of direct spread by the organisms, and that a systemic infection by the blood stream could account for conditions apparently so remote as endocarditis, arthritis, pleurisy and peritonitis.

It is not necessary in these days to labour the infectivity of the disease in women; but one still finds many remnants of the old confused ideas prevalent in practice; and it is owing to the persistence of these old fallacies, still half held, that we owe the laxity yet present in the standard of cure insisted upon, particularly in women. In the male detection of abnormal symptoms is comparatively easy. The patient himself can see, or feel, in most cases that something is wrong. But in women this is not so. The difficulty produced by leucorrhœa, or what the older writers called "fluor albus," is always present when the question of cure arises. It is a difficulty that puzzled every writer in the past before the causal organism was known. It is a difficulty that still confronts all clinicians, for it can be asserted positively that there is as yet no

positive naked eye criterion of cure. When all clinical signs have gone the patient may still be infective; and every favourable opinion, therefore, that we give must always be strengthened and confirmed bacteriologically, if it is to be of any value.

Tests for Cure

Turning now to the practical side of the question, we have to ask ourselves what steps can be taken to ascertain that any given individual, who has had the disease, is no longer capable of infecting his or her partner in conjugal relationship?

In the past we have seen that standards have been promulgated, with all the weight of professional opinion behind them, which have proved not only erroneous but positively dangerous. It is quite possible that some of the standards proposed to-day will appear extremely crude to those who follow us. But each generation must legislate for its needs according to its knowledge; and so, with the humility of one who has studied the errors of the past, I propose to offer mine for your criticism. The complexity of the subject is ever increasing, and the standards of even ten years ago now appear too lax to us—new tests, new instruments of precision constantly tending to raise the stringency of the index of cure.

The Case of the Male

The first and most obvious test is the absence of urethral discharge in a morning urine held for some four hours. This urine must be clear, and the centrifuged specimen free from pus cells and organisms. I wish to accentuate the word "organisms" rather than gonococci, for I consider the presence of other organisms almost as dangerous as the gonococcus itself. Many urologists insist on the absence of threads, but if repeated examination of such threads proves them free from pus cells and organisms when stained, and sterile on culture, I think they may be neglected. There is a condition of "mucorrhœa" due to over-activity of Littré's glands started by the irritation of the disease. This mucorrhœa, declining slowly, may continue for some months after infectivity has passed away. Some urethras, moreover, have been

so damaged by over-irrigation and over-instrumentation, before one sees them, that they never recover to normal, and their unfortunate owners may therefore pass threads for the rest of their lives. If threads, then, show pus cells or organisms of any kind the patient is not cured. If not, on repeated examination, they may be ignored as a symptom.

Exercise

When the patient's urine has been clear for a fortnight he may be allowed exercise. There is nothing that brings back a discharge so readily as muscular activity. I have found it of enormous value to interdict any form of exercise such as riding, rowing, dancing, working a sewing machine, in out-patients. Amongst the Metropolitan police I have learnt that a too speedy return to duty brings on turbidity and pus in the urine almost at once. Exercise, therefore, can be used as an excellent indicator. If this does not cause any return of discharge, massage of the anterior urethra on a straight sound should next be tried. Any mucus at the orifice, or turbidity in the urine, can then be tested bacteriologically. After this comes massage of the posterior urethra on a flexible bougie, and a similar examination of the meatal drop, and of the urine can be made in smear and culture. Whilst these investigations are going on, one will have satisfied oneself that there are no clinical evidences of disease in the vesicles, prostate, cord, testes, Cowper's glands or anterior urethra.

Provocative Vaccines

Next a provocative vaccine should be given, and the discharge or urine examined twenty-four, forty-eight and seventy-two hours later. I use 1 c.c. of the Lock Hospital non-toxic vaccine, but a gonococcal proteose is equally good. If all these tests are negative I repeat them at the end of three months without treatment, and then examine with the urethroscope.

The Urethroscope

Until a patient has been clear of gonorrhœa for three months I do not consider a urethroscopic examination will show a normal urethra. I therefore do not recommend its systematic use as a test of cure in uncomplicated cases

until other methods of examination have been exhausted. The urethroscope should be reserved for the examination of cases that do not clear up, recurrent cases, cases of suspected stricture, etc. It should never be used in the acute stages of a gonococcal attack, as irreparable damage may thus be done. A safe rule to adopt is never to use the urethroscope in any case where free pus is present in the urine, unless it is absolutely necessary for diagnostic purposes.

Three months later I again repeat the foregoing tests.

You will notice that I do not use the "beer test" or the silver nitrate provocative test. I particularly dislike the silver nitrate test. I have seen too many cases where it has caused serious damage to the urethral mucous membrane ever to use it again. Unfortunately it was one of the tests recommended by the Ministry of Health [28] in 1919, and on that account has been used extensively. I am all the more pleased to hear, therefore, that it is no longer employed by the majority of venereal experts in London to-day. It was one of our mistakes and may decently be interred.

The Case of the Female

Here the problem is much more complicated than in the male, for two reasons: First because it is almost impossible clinically to say when a woman is cured, and, secondly, because pregnancy introduces complications which mask symptoms considerably. The difficulty in diagnosing cure need not be stressed. Every one admits it. The fact that experts in the past have not only been entirely ignorant of many of the more serious complications, but also unaware of the infectivity of cervical discharge, indicates how great the difficulty must have been before the causative organism was known. Our trouble now is that, even with the most elaborate technique, gonococci actually present in the cervical discharge may still be missed. The anatomical peculiarities of the female, moreover, provide numerous other lairs for the lurking organism. The patient herself cannot inspect the parts, as in the male. Pain after the first few days is not a striking feature in most cases. The presence of the comparatively innocuous, and almost physiological, leucor-

rhœa adds to the difficulty. Every test, therefore, that can possibly be used should be used, because it is only on the cumulative evidence of them all that one can venture to give a certificate of non-infectivity. As I have pointed out elsewhere, there are only two infallible testing media for the presence or absence of gonococci in the female. These two media are the mucous membrane of the male urethra and the conjunctiva of the child. They will find the gonococcus when all our elaborate examinations fail; and, unfortunately for us, it is on the results of tests thus made by Nature that all our artificial safeguards will be judged.

I have indicated in the *Lancet*,[29] and in my book on Gonorrhœa in women,[30] what, in default of the real tests indicated above, we can use; and I will recapitulate them here, with the modifications suggested by later experience.

First. All clinical signs of the disease must have disappeared. By this I mean that there is now no induration in Bartholin's glands, and no signs of inflammation at their orifices. That the urethra similarly appears normal on inspection, palpation from the vagina, and urethroscopic examination. That the paraurethral crypts, especially Skene's ducts, show no signs of inflammation. That the introitus is normal, and no inflamed tags or crypts are present. That the cervix appears normal, the secretion clear and no erosion is present. That the uterus and tubes on bimanual examination appear free from disease. That the anal orifice seems normal, and there are no signs of inflammation or discharge on passing a speculum.

The second essential desideratum is that all bacteriological evidence of disease should be absent. By this I mean that smears taken from the urethra after massage should be negative on microscopic examination and on culture, before and after a provocative injection of gonococcal vaccine or proteose. That the urine should be clear and free from pus cells or gonococci in a four-hour specimen. That smears from Bartholin's glands and Skene's ducts after massage should be negative. That smears from the cervix should be negative in culture and on microscopic examination, when taken before and after menstruation, and after a provocative vaccine. That smears from the anal orifice should be negative.

SMEARS *versus* CULTURES

I have found smears more useful than cultures in detecting the presence of the gonococcus in the female, but I gather that others have had a contrary experience. I have therefore come to the conclusion that in a disease in which the diagnosis of cure is so admittedly difficult, one is not justified on personal grounds in neglecting any method of testing that others have found reliable. It is sometimes possible to get a positive result by smears that has not been obtained by culture. It is also possible to find the gonococcus in culture when it has been missed in smear. One positive result invalidates a hundred negatives. No method therefore of arriving at a reasonable certainty should be neglected.

RELAPSES IN THE FEMALE

Every test enumerated above should be repeated in three months, six months and twelve months after apparent cure. The disease in women has a tendency to recur, even under the best hygienic conditions. This has been our experience at the Lock Hospital, where there is a Rescue Home to which girls declared cured are admitted. There is a very definite relapse rate even in these carefully inspected cases, and my experience therefore is that no patient can be pronounced cured until a period of at least eighteen months free from symptoms has elapsed.

THE COMPLEMENT-FIXATION TEST

And now, in conclusion, I come to the serological test. Here my remarks apply equally to either sex. Theoretically the test is a most attractive one. If a positive C.F.T. for the gonococcus meant that the patient had the disease, and a negative C.F.T. that he or she was free from it, all the elaborate paraphernalia of tests would be jettisoned at once. But, alas, it is not as simple as this! Practically the test has many drawbacks. It is generally stated that the reaction persists in diminishing strength for some months after the patient is cured. It will be found that, if the case has been treated with an efficient vaccine, the time the reaction takes to become negative after cure is considerably prolonged. It is generally believed that the presence of a positive C.F.T.

indicates that the patient has had the disease. This, however, is not absolutely so, for if a case which is not gonococcal is treated with a course of gonococcal vaccines he or she will give the reaction for some time afterwards.

Is a Persistent Positive C.F.T. for the Gonococcus a Bar to Marriage?

It would seem probable that the presence of a persistent C.F.T. for the gonococcus, without active discharge, is an indication that the patient has a focus, probably quiescent, but still capable of activity in the body. This focus may be a nodule in the tail of the epididymis, a closed ovarian tube or seminal vesicle, a quiescent gonococcal joint, a nodule in the prostate shut off from the posterior urethra, etc. Such a focus may light up an iritis many years later, keep up a gonococcal rheumatism, or, in certain adverse conditions of health, precipitate a malignant endocarditis.

Should such a patient be precluded from marriage? Yes, in certain conditions; but No, in others. To my mind it is simply a question of the risk of infectivity. A woman with a closed tube, or a joint, whose cervix and urethra is clear, is not likely to infect her partner. There are thousands of cases of women with tubes marrying again without apparent consequences. They are sterile, and that is their main disability. But they do not infect their husbands. Men with the nodule of an old epididymitis marry cheerfully without disaster. Only very occasionally does such a nodule light up again, and then, as a rule, owing to a later re-infection. Each case, then, must be judged on its merits. We are not discussing the "Cure" of gonorrhœa, but "Gonorrhœa and Marriage." If, then, the patient's focus is in such a position as to preclude the likelihood of infectivity, he or she may be allowed to marry, the comparative small risk of a possible recrudescence having been pointed out beforehand. A persistent C.F.T. for the gonococcus is not, therefore, *ipso facto*, in my opinion, necessarily a bar to marriage.

Is a Negative C.F.T. for the Gonococcus an Indication that Marriage is Permissible?

Here, again, one cannot give an absolute answer. If there is a negative C.F.T., and all signs of the inflammation

that suggested the test have disappeared, it would seem obvious that there are no risks to a safe marriage as far as the gonococcus is concerned. But a negative C.F.T. may exist under three conditions, which make one very anxious not to give too hasty an assent. The first is that the test is not positive until some weeks after the disease has become definite clinically. In this case the obvious discharge would in itself debar marriage, and one may dismiss the question at once. But there is evidence accumulating that quite a considerable number of patients never give a positive C.F.T. after their gonococcal infection. Such patients are subject to recurrent attacks of the disease, in an acute form, without their having risked re-infection. They have such a poor resistance to the organism that they do not develop antibodies sufficient to produce the reaction. These people are extremely dangerous to others, as they apparently clear up only to relapse again six months, twelve months, two years later, generally with an acute joint or an iritis accompanying a return of urethral discharge. Any intercurrent illness seems to precipitate a recrudescence of the original complaint. If one depended on a negative C.F.T. in such cases, not knowing the history, one would inevitably be courting disaster. The third type is that of the patient with a negative C.F.T. and a persistent mild staphylococcal or other secondary organism infection remaining in the urethra. Such patients are still highly contagious, although they give a negative C.F.T.; and if you assure them they are free from the gonococcus, and allow them to marry, the chances are they will set up an acute inflammatory discharge in the partner. If your patients get an acute urethritis or endocervicitis from such secondary organisms, after you have told them they are free from the gonococcus, you will find it very difficult to persuade them that you did not make a fatal mistake in allowing them to marry or resume conjugal relationship.

This is not the place to discuss the dangers that may arise from a secondary organism infection which persists after the original gonococcal one has cleared. That is an enormous field of its own. There is one thing, however, I would like to impress on you, and that is that if you wish your patient to be safe you should make up your mind that *a negative C.F.T. for the gonococcus must be*

accompanied by a complete absence clinically of all signs of inflammation before you can sanction sexual intercourse.

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